Everett from
Capt. Smyth

STORY

OF

THE NEW PLANET, NEPTUNE.

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STORY OF THE NEW PLANET, NEPTUNE.

"What if the Sun
Be centre to the world; and other stars,
By his attractive virtue and their own
Incited, dance about him various rounds?
Their wandering course now high, now low, then hid,
Progressive, retrograde, or standing still,
In six thou seest: and what if seventh to these
The planet Earth, so stedfast though she seem,
Insensibly three different motions move."

THUS sung Milton in the middle of the Seventeenth century, when Astronomers were contented with six primary planets besides the terrestrial globe, which was held by mankind to be the centre of the system: since that period, no fewer than seven more of these revolvers have been detected, and 'tis a joint-stool to Cassiopea's chair that others will still be found. Thus the recent triumph of science in the *à priori* pointing out of such a body, because certain twitchings in the motions of Uranus indicated an exterior attraction, is an advent so unexampled as to form one of the most brilliant achievements of intellect ever recorded, and to raise prospects of future harvests. Its story must therefore be placed on our pages.

It will be recollected that Kepler, looking to the general harmony of the celestial spaces, and the obvious analogy which existed in the distances of the primary bodies from the sun, had already confidently predicted the discovery of a planet between Mars and Jupiter. This notion, though not resulting from any known law of nature, seized upon men's minds; Lambert also considered another body as necessary in the construction of the universe; and Bode, in 1772, found that something was required to make the table, which he had analogically constructed, accommodate itself to the system. The actual discovery of the more distant Uranus nine years afterwards aroused attention to the hypothetical subject, and in 1789, Baron de Zach confidently published in the Berlin Almanac for that year, the elements of the orbit of the yet undiscovered planet which ought to be found in the vacant space, making its assumed distance from the sun 268 millions of miles, and its period 4 years and 9 months. In 1800, an association of 24 astronomers was formed for prosecuting the search by observing every

visible zodiacal star. After the accidental finding of Ceres in 1801, having nearly this very distance and period, Professor Bode communicated his celebrated empirical law of the planetary system, above alluded to, to Baron de Zach. This simple law is so far a mere experiment, that no rational ground or physical theory can be adduced in its favour, nor is it capable of mathematical demonstration; and yet it has had the proud triumph of achieving wonders in respect to the Asteroid family, and has now formed the basis of the heliocentric distance assumed for the new exterior planet. The scheme teaches, that the interval between the orbits of any two planets is about twice as great as the inferior interval, and only half the superior one. In other words, the excess of the distances of the planets above Mercury form a geometrical series, of which the converse ratio is 2. Now, if the radius-vector of Mercury is represented by 4, then that of the other planets will follow the annexed law of distance, increasing by 3×2 and its powers; and this very ingenious relation is thus marshalled:

Mercury	$4 = 4$
Venus	$4 + 3 = 7$
Earth	$4 + 3 \times 2^1 = 10$
Mars	$4 + 3 \times 2^2 = 16$
Ceres (as mean of Asteroids) ..	$4 + 3 \times 2^3 = 28$
Jupiter	$4 + 3 \times 2^4 = 52$
Saturn	$4 + 3 \times 2^5 = 100$
Uranus	$4 + 3 \times 2^6 = 196$
Neptune	$4 + 3 \times 2^7 = 388$
Hypothetic Planet	$4 + 3 \times 2^8 = 772$

and so on to the 10th power, beyond which there can be no hope of a visible body. And it is not a little singular, that this most accidental tabulation should yield nearly the correct relative heliocentric distances of the primaries of our system.

To be sure, the newly-found planet seems to be six or seven solar radii short of what Bode would give; but it must be recollected that something may still be shown for this, there being certain little items remaining for philosophy to ponder upon. The mean distance, from a very rough view of the few observations hitherto taken, may be considerably under 32 of those radii: but it should be borne in mind that the empirical distance used, led to the grand discovery which we now treat of. At all events the said distance is not so violently out, but that deeming it an utter failure is *troppo duro*. This curious law has already achieved much; and another celestial wanderer may yet be caught by it, since astronomers and geometers are now rowing together. They say that Titius, a Wittemberg professor, gave Bode the

first notion of it. Titius is known to have obtained his idea from a musty old work; and we verily believe it would be traceable even to Kepler himself, the very Coryphæus of planetary inquirers, as well as the most likely man in the world to have framed such a scheme; indeed it seems to bear his signet. Be this, however, as it may, we repeat that we cannot altogether quadrate with those who deem the present application of the so-called law as entirely a miscarriage. In this instance, according to what we can yet scrape together, the mean distance is actually found to be much less than that which was assumed in computing the residual perturbations; but it also seems, that the case of the two exterior bodies maintains a very near approach to the ratio of two to one in their times of revolution—that is, the year of Uranus being 84 of ours, one is pretty safe in concluding that that of Neptune will prove to be somewhere about 170 mundane years. But we are anticipating our story.

Few of our readers are unacquainted with the *empirical*, though philosophical discovery of the Asteroids, as they were prepensely sought for in a region where, according to the best guesses, a planet ought to have existed; and where, upon a singularly strict and methodical scrutiny, four little planets were actually found—Ceres, Pallas, Juno, and Vesta—which, from several very receivable evidences, have been inferred to be mere splinters of a large body which has undergone some tremendous catalysis. They all conform to the same order of distance—two of them with almost the same periodic times—and the orbits of all of them, so far as known, intersecting each other in a very unusual manner. This is startling, seeing that every other distance on the scale of the Solar System, gives but one primary orb, albeit there are satellites in four of the cases. Lagrange and Olbers thought that such ruin might be effected by a force with a velocity only 20 times that of a 24-pound shot on first leaving the mouth of a gun. From this great probability of a ruined planet, it was considered that other fragments would still be occasionally picked up; and accordingly Herr Hencke, of Driessen, in Prussia, on the 8th of December, 1845, discerned a fifth small asteroid belonging to the same group, which has received the name of Astræa.

Great, however, as was the triumph of mind and means in the discovery of this new planetary family, the finding of the Asteroids has just been so greatly transcended, that it must now occupy a secondary niche in the temple of Physical Astronomy. By methods depending on the furthest advance ever yet made in theoretical and practical thought and science, a new body of the Solar System has been brought to light; and while the wondrous tidings are still ringing in our ears, a few words may be acceptable even though we may perchance pay out a little stray line.

The doctrine and admirably successful application of mathematical analysis to the profound study of celestial phenomena, have opened and proved the perfection of the scientific character of Astronomy, and the preponderating importance of the fixed correspondence of cause and effect, which it unveils. This surprising mechanism, exhibited by the laws of the planetary motions, may be justly considered as the grand foundation of the whole system of positive knowledge: and yet, in spite of appearances, the phenomena are, in fact, much more simple than would be thought. The most complicated problem which they present—that of the modification produced in the motion of two bodies tending towards each other by their mutual gravitation, from the influence of a third acting on both in the same manner—is less complex than some other portions of inorganic physics; and yet it presents such difficulties that the solutions of it are still only approximative. The Solar System has presented certain facilities for advancing to perfection in the unravelling of its several problems; and each hypothesis admits of numerical verification. The planets of which this system is composed being few in number, having their masses very unequal, and incomparably smaller than that of the sun: their forms being almost spherical, and their orbits—slightly inclined towards each other—and nearly circular. From these circumstances, it results that the perturbations are often very slight, and that in order to calculate them it is commonly sufficient to take into account, concurrently with the action of the sun upon each, the influence of one other planet, capable, from its magnitude and proximity, of producing sensible derangements. These disturbances had heretofore given philosophers no small uneasiness, because they were not understood, and from what was obvious to the mere senses, there appeared to be a general tendency in the planetary bodies of the Solar System to descend to the common centre of gravity of the whole. Now, under the potent subjunctive, *if* this were true, it would follow that the material universe could exist only for a few millions of years longer. Such a catastrophe was truly alarming in anticipation; and in the general dismay which it occasioned, the tuneful bard of the Botanic Garden thus sighed—

> "Roll on, ye stars, exult in youthful prime,
> Mark with bright curves the trackless steps of time;
> Near and more near your beaming cars approach,
> And lessening orbs on lessening orbs encroach.
> Flowers of the sky! ye too to age must yield,
> Frail as your silken sisters of the field!
> Star after star from heaven's high arch shall rush,
> Suns sink on suns, and systems systems crush:
> Headlong extinct to one dark centre fall,
> And death, and night, and chaos mingle all!"

But hope was quickly at hand, the conditions of the case were closely scrutinized, and the geometer, extracting a gratifying destiny from his formulæ, chaunted a palinody, and declared nature to be immortal! In short, the noble brace of *compatriotes*, Laplace and Lagrange, after submitting the celestial perturbations to the test of the extraordinary resources yielded by the Differential Calculus, went far to demonstrate the eternal duration of the universe: in fact, they fully showed that the effects of pure gravitation-disturbances in the Solar System, went on compensating one another. Nor was this all: the law of those changes, which prescribes to each its limits, and forces it to recur in endless repetitionary series, was clearly established. This was extremely satisfactory on the whole; but there are still misgiving inquirers, who think that a resisting medium, and other natural practical difficulties may exist, which are not taken account of in the mathematical theory of gravitation. This, however, is but a mare's nest as yet.

Such, then, is the general argument. All the planetary motions are affected by the gravitation of the planets towards one another; and their places in the heavens are computed beforehand, so that the positions given by observation can be constantly compared with those previously calculated. This led to the recent extraordinary revelation. The observed motions of Uranus, the most distant planetary body hitherto known in our system, when thus compared, were found not to agree with those which it ought to have had after allowing for the influences of all the known planets. In 1821, Mons. Alexis Bouvard published his Tables of Uranus, founded on a continued series of observations extending from 1781, the year in which the elder Herschel detected it, to the date of that publication. Previous, however, to its discovery as a planet, it had accidentally been observed no less than seventeen times as a fixed star, the earliest being in 1690, by Flamsteed. When Bouvard was constructing his tables, he attempted to form them as well on the older as the later observations, but whatever ellipse he ascribed to Uranus, he could not make the theoretical agree with the observed course of the planet; and thinking these discrepancies might be owing to imperfection of method and means in former days, he rejected all the observations previous to 1781. It was afterwards found, however, that the rejection had been made on insufficient grounds, and that, from some unknown cause, the theory itself was in fault, the difference amounting to no less a quantity than 96″ of geocentric longitude in 1841. When it was thus found that the deviations were far greater than any which could be ascribed to mere errors of observation—that they were of a regular character—that they were in the same planetary direction—and were of such a nature as would arise from the action of a still more distant planet, attention was naturally directed to inquire whether the dis-

turbances were such as to afford a clue, which might possibly lead to the location of the disturber, whose attraction was thus influencing the progressive motion of Uranus. This—after much labour and many misgivings—betrayed the attractor; for it seemed quite clear that, without a departure from the received law of attraction, or the actual existence of a massy disturbing body, there was no accounting for the irregularities under notice. The whole, however, was held to be a mere transient hypothesis until within the last handful of years; and this brings us to our eventful story.

Among all the physical sciences, that which investigates the laws of the celestial bodies, and extends man's views to other spheres, is unquestionably the most beautiful effort of the human mind: and it follows that the recent installation of Neptune—the third largest planet of our system—is among the noblest feats of Astronomy. This we confidently premise, since the predicting and finding were not the fruits of accidental fortune, persevering scrutiny, nor telescope power; but the magnificent consequence of a judicious exertion of profound thought and transcendental computation. Indeed, in the whole history of science, there is no event more striking than this, and its circumstances are so interestingly curious, that we beg our readers' close attention to the features of the case.

The Astronomer-Royal, having been somewhat officially made acquainted with many of the leading circumstances, gave a succinct historical detail of them at the Meeting of the Royal Astronomical Society of London, on the 13th of November, 1846. Throughout this interesting discourse, there is internal evidence of the strictest truth, and it furnishes a mass of circumstantial facts, which no mere opinions can rebut. He commences with observing, that the irreconcileability of the motions of Uranus with the law of gravitation gained general credence from the publication of the French *Tables of Uranus* in 1821; and he afterwards adds—"I know not how far the extensive and accurate calculations of M. Eugène Bouvard may have been used in the subsequent French calculations, but I have no doubt whatever that the knowledge of the efforts of M. Bouvard, the confidence in the accuracy of his calculations, and the perception of his failure to reconcile in a satisfactory way the theory and the observations, have tended greatly to impress upon astronomers, both French and English, the absolute necessity of seeking some external cause of disturbance." In November, 1834, Mr. Airy received a letter from the Rev. Dr. Hussey, who had recently visited Paris, in which he remarks on the possibility of a disturbing body outside Uranus; but adds that he "found himself totally inadequate to the task" of hunting it up, so as to determine its position even approximately, or empirically. He therefore relinquished

the matter altogether; but on mentioning it to Bouvard, that astronomer said it had occurred to him also*—and that Hansen conjectured there were two unseen planets in the case, as one disturbing body would not satisfy the phenomena.

This is the Astronomer-Royal's first point of departure; and the act of publishing his own reply, in which he doubts the possibility of making such a determination, is creditable to his candour. As, however, straws thrown up often serve to show which way the wind blows, we are surprised at the omission of many other existing hints on the subject, for even dubbing them only *fancies*, they all tend to show that the discovery of Neptune is fairly a result of the march of thought, consequent on the movement of the age. Now everybody recollects that Kant predicted, or rather demonstrated, that a planet exterior to Saturn would be found, as a deduction of certain laws which he had established in his *Himmels' System:* for he suspected that, as nature does not ordinarily proceed *per saltum*, the system of planets must pass by *gradation* into the system of comets. Therefore, he asserted, at some future period, there will be found *at least one planet* superior to Saturn—whose orbit will be much more excentric—and will thus supply a link to connect the motions of the planets and the comets into a more continuous chain. This moral, rather than mathematical, decision was given at least 26 years before Herschel made his discovery. In 1758, Clairaut also supposed the irregularities of Halley's comet might be owing to planets too distant to be ever perceived by us; and a floating notion to that effect is traceable elsewhere. There is a passage somewhat in point in J. G. Jacobi's Pocket-book for 1802, which runs thus—"Ophion, the next planet beyond Uranus, is 780 millions of miles distant from the Sun, and has an orbit of 250 years. It is not yet discovered." Cacciatore, of Palermo, must have had notions of a similar stamp, since we find him writing to Captain W. H. Smyth, in 1835, that he had watched a *suspicious* star near No. 17 Hora XII. of Piazzi's Catalogue, and found a movement of 10″ in right ascension in three days: "So slow a motion," he said, "would make me suspect the situation to be beyond Uranus†."

Mrs. Somerville, in the third edition of her *Connexion of the*

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* In 1837, Eugène Bouvard wrote to Mr. Airy,—"Cela tient-il à une perturbation inconnue apportée dans les mouvemens de cet astre par un corps situé au-delà? Je ne sais, mais c'est du moins l'idée de mon oncle."

† It having been publicly advanced that this stranger and Le Verrier's might prove to be identical, we beg to remind the reader, that the latter has only 16° of motion in 10 years; whence, from its longitude in 1835, it was then in the XXth Hour, or 120° from the scene of Cacciatore's phenomenon. Nor has Wartmann's possible planet of 1831, any stronger claims to relationship with Neptune.

Physical Sciences, 1836, at page 74, says:—"The tables of Jupiter and Saturn agree almost perfectly with modern observations; those of Uranus, however, are already defective, probably because the discovery of the planet in 1781 is too recent to admit of much precision in the determination of its motions, or that possibly it may be subject to disturbances from some unseen planet revolving about the sun beyond the present boundaries of our system. If, after a lapse of years, the tables formed from a combination of numerous observations should still be inadequate to represent the motions of Uranus, the discrepancies may reveal the existence, nay even the mass and orbit, of a body placed for ever beyond the sphere of vision." In 1841, Herr Mädler published his excellent volume *Populäre Astronomie*, in which—discussing the probability of finding new planets—he says:—

"Finally there remains the unlimited space beyond Uranus. The domain of the Sun extends at least forty times farther, for comets such as that of 1680, attain this distance in aphelion, and many of those whose orbits can only be now reckoned as parabolas, may extend considerably beyond. From what we know of the distance of the fixed stars, the nearest is distant several hundred thousand times that of the Earth from the Sun: therefore a planet whose distance is only two thousand times that of the Earth ($= 100 \times$ ♅) would not incur a perceptible perturbation from the nearest fixed star, should this not much exceed that of the Sun. The existence of still more planets beyond the Uranian orb, is for these general reasons already very probable.

"Special reasons may be added in support of this probability, the most important of which is the following.

"If a planet revolves beyond ♅, it must (if its mass be not very small) act on ♅, producing anomalies in the latter's course, inexplicable unless the disturbing body be known. Of course these anomalies would only appear after a considerable time, for by reason of the ♅ period of 84 years, and the still longer one of the disturbing planet, these effects remain nearly the same for a long series of years, are mixed up with the elements deduced from the observations of those years, and are practically inseparable from them. But calculating a considerably earlier series, *other* elements would result, and one cannot combine both series in one system of elements without leaving notable errors. This is really the case with ♅. Bouvard (Tables Astr. de ♃, de ♄, et de ♅, Paris, 1821) found the pre-Herschelian observations incompatible with those elements which the more numerous observations from 1781 to 1820 gave, and which we must assume, on account of the latter ones being preferable. The deviations are by no means so great as to make us doubt the observed body having been ♅, but too great to be reputed errors of observation on the part of such careful astronomers. Moreover the observations after 1820 deviate considerably from Bouvard's tables. Airy has shewn from the oppositions of 1833 to 1837 that ♅'s radius-vector for these years differs from the tables by a quantity exceeding

the distance of the Moon from the Earth, and therefore certainly the last twenty years calculated by themselves would give quite a different system of elements to the preceding forty years. Did we *not* take into account ♅'s perturbations of ♄'s orbit, or ♄'s of ♃'s orbit, we should find similar deviations, and had we possessed *very exact* ♄n observations for a long series of years, it would have been possible to have discovered ♅ *theoretically* by analytical combinations before Herschel found it, presuming that all the disturbing masses were sufficiently well known and introduced in the computations.

"Applying this conclusion to a member beyond ♅, we approach (es liegt nun nahe) a *planet acting upon and disturbing it;* yes, we may express the hope, that analysis will some time or other solemnize this her highest triumph, making discoveries with her *mind's* eye in regions where our actual sight was *unable* to penetrate.

"Still the latter is not absolutely true. Although ♅ is considerably fainter than the other old planets, it is yet far from the extreme limit of visibility; a sharp naked eye may perceive him. A planet, whose brightness would be to ♅'s, as ♅'s to ♄'s would not be the faintest object in great telescopes, especially if its diameter be not too inconsiderable. The small orbital inclination of the three exterior planets to the Ecliptic, renders it probable that the one or more to be discovered do not exceed them by above 1° or 2°, and a few years' continuous investigation of the Ecliptic and its nearest limits would be at first the most probable means of realizing this hope, or otherwise of shewing that there is no planet beyond ♅ to which the optical power applied was sufficient."

The author then proceeds to Wurm and to Bode's law $a+b^{n-1}c$, to which he however attaches little credit, and says that although this law might have prevailed primitively, the *present* Solar System requires perhaps the introduction of the masses, inclinations, and excentricities: the latter being subject to secular variations, and our being ignorant of the *age* of the Solar System, render Bode's series imperfect; if true, we should have $a_8 = 38{\cdot}8$, $T_8 = 243$ years; $a_9 = 77{\cdot}2$, $T_9 = 7$ centuries, &c. He speaks of its having anticipated the Asteroidal discoveries, and concludes with the circumstance of the place of the aphelion of the comet of 1680 from Bessel's calculations.

(Table p. 346, No. 36 $= 2 \times 426{\cdot}6858 - {\cdot}00622236 = 853{\cdot}3654$; $T = 8813{\cdot}782$ years) allowing of five planets, the last of which has $a_{12} = 620$, $T_{12} = 15000$ years.

Such hints may be deemed vague and indefinite, yet they ought to have been cited at a general muster, as bearing on the upshot; and there is still a point of greater interest and exactness, which should have been noticed. Early in the month of July, 1842, the illustrious F. W. Bessell visited Sir John Herschel at Collingwood, when, among other matters, he remarked that the motions of Uranus, as he had satisfied himself by careful examination of the recorded observations, could

not be accounted for by the perturbations of the known planets; and that the obliquities far exceeded any possible limits of error of observation. "In reply to the question," says Sir John, "whether the deviations in question might not be due to the action of an unknown planet?—he stated that he considered it highly probable that such was the case,—being systematic, and such as might be produced by an exterior planet. I then inquired whether he had attempted, from the indications afforded by these perturbations, to discover the position of the unknown body,—in order that a *hue and cry* might be raised for it. From his reply, the words of which I do not call to mind, I collected that he had not then gone into that inquiry; but proposed to do so, having now completed certain works which had occupied too much of his time." A few days after this visit to Herschel, at a breakfast with us in London, when we expatiated on the beautiful coincidence of lively and even fanciful theory with industrious practice afforded by the Asteroid plot, he remarked—"There may yet be something still more so, for what if another planet shall be found outside Uranus?" We replied that should it observe Bode's law, it would be too distant for vision: "That," rejoined he, "must depend on its volume." With a view of attacking this grand problem, on his return to Germany, he engaged the services of M. Flemming, to make all the necessary reductions of the available observations of Uranus; the labour was well advanced, but poor Flemming died, and Bessel himself was attacked by the fatal illness which interdicted every abstruse mental application.

We have now approached the time when analytical investigation led to a distinct indication of the place where the disturbing body ought to be sought; and the wondrous efforts by which it was accomplished are confined to two geometers—Le Verrier and Adams. We will proceed with the former, as his views were first published to all Europe, and his operations claim, therefore, priority;—merely premising that each, in ignorance of the other's labours, proceeded to investigate this most intricate question, and arrived independently at all but the same conclusions, for both pronounced that the probable place of the suspected planet was about 325° of heliocentric longitude!

In the *Comptes Rendus* of the French Academy, 10th of November, 1845, there is a most valuable Memoir, by M. Le Verrier, on the perturbations of Uranus produced by Jupiter and Saturn; and on the errors in the elliptic elements of Uranus, consequent on the use of erroneous perturbations in the treatment of the observations. Herein the secular inequalities of those superior bodies are more rigidly investigated than ever, and many small terms—*un certain nombre de petits termes qui n'avaient pas été donnés*—added to the conditions; but on a laborious examination of the correction for the new elements, it was found to be

incapable of explaining the observed irregularity of Uranus. "Cette discordance," he says, "préoccupe vivement les astronomes, qui ne sont pas habitués à des pareils mécomptes. Déjà elle a donné lieu à un grand nombre d'hypothèses. On est même allé jusqu'à mettre en doute que le mouvement d'Uranus fût rigoureusement soumis au grand principe de la gravitation universelle." And he sums up,—"Tel est effectivement le sens de l'erreur des Tables actuelles. Seulement, l'écart est plus considérable, et le surplus peut tenir à d'autres causes dont j'apprécierai l'influence dans un second Mémoire."

The promised memoir was published in the Comptes Rendus for 1st June, 1846, and though rated by its author only a sketch—*recherche*—was received with delight and astonishment by the scientific public, who seemed to partake of the geometer's zeal and confidence. M. Le Verrier re-considers all the possible conditions of the wandering between theory and observation in the orbit of Uranus, and, on grounds of the strictest geometrical reasoning, concludes that none of the explanations offered—as the resistance of ether, decay of gravitating power, action of a vast satellite, or the shock of a comet—are admissible, save that of an unknown planet. On this supposition he showed that if this be the cause of the disturbing force, the stranger must be, not *within* the orbit of Uranus, because, if a large body, it would trouble the path of Saturn also; if a small one it would be inadequate to produce the discerned effects; nor, for the same reasons, near on the outside of Uranus: but far enough *without* the orbit of the latter to act upon it, without acting upon that of Saturn; and large enough to act upon Uranus for long and continuous periods of time. The great question now was—where then is this body situated, what is its mass, and what are the elements of its orbit*? The great difficulty here encountered was, the uncertainty relative to the precise ellipse described by Uranus; because according to that orbit's variation, the stranger's supposed influence must be varied. It is requisite to form the expressions of the perturbations due to the hypothetical body, in functions of its mass and of the unknown elements of the ellipse which it describes: these perturbations must be introduced into the co-ordinates of Uranus, computed by means of the unknown elements of this planet's orbital ellipse. Equating the co-ordinates thus obtained to those observed, the elements

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* Perhaps the reader will here like the *ipsissima verba* of M. Le Verrier, since they are as remarkable as terse:—"Nous sommes ainsi conduits à nous poser la question suivante: *Est-il possible que les inégalités d'Uranus soient dues à l'action d'une planète, située dans l'écliptique, à une distance moyenne double de celle d'Uranus? Et s'il en est ainsi, où est actuellement située cette planète? Quelle est sa masse? Quels sont les éléments de l'orbite qu'elle parcourt?*"

of the ellipse described by both planets, must be regarded as unknown, in the equations of condition which result therefrom. Then rigorously eliminating the orbital elements of Uranus, we obtain the relations between the stranger's mass, the excentricity of its orbital ellipse, and its mean longitude at the commencement. These new relations determine the expressions of the orbit's excentricity, and the longitude of the perihelion, in functions of the mass, and of the longitude of the epoch. Finally, only the mass of the planet and the mean longitude at the original time will remain arbitrary; and these must be selected so as to suit the rest. Such are hard conditions, my masters: but Le Verrier proceeds under them to investigate such an orbit, assuming a mean distance suggested by Bode's Law, and concludes by pronouncing as the most probable result of his investigation, that the true heliocentric longitude of the disturber will be about 325° for the 1st of January, 1847, confidently affirming that an error of 10° in this place is not probable.

Never was a bolder paper than this thrown out to public scrutiny; and it was deemed conclusive, although it gave but a single element, without affording results respecting the stranger's mass, or the form of its orbit. But in the *Comptes Rendus* for 31st August, 1846, M. Le Verrier communicated a third paper, in which he proceeded to fix more exactly the place and distance of the yet unseen planet, the size being pronounced on a pure hypothesis respecting its density. He now distinctly described it as a body many times the magnitude of the earth, and not much less than Saturn, taking more than two centuries to revolve about the Sun, at a distance 32 times greater than that of the Earth. To obtain the quantities to substantiate these views, he grouped all the observations into 33 equations, explaining the peculiar method by which he derives the values of unknown quantities from them: and these are the elements which he thus acquired for the 1st of January, 1847—

Mean Longitude	318° 47′
Perihelion	284° 45′
Semi-axis major	36·154
Excentricity	0·10761
Periodic time, sidereal years ..	217·387
Mass (Sun as unity) $\frac{1}{9322}$ ..	0·0001075
True heliocentric longitude ..	326° 32′
Radius-Vector	33·06

Within one month after the Philosopher had thus minutely fixed and published beforehand the place of this mysterious body, and the limits between which its *locus* must be sought, it was actually *bagged!* In a letter received at Berlin on the 23rd of September, M. Le Verrier

urged Dr. Galle, of the Observatory at that place, to sweep sharply for the new star, which he expected would be recognised by its disc. That very evening Galle repaired to his post, and on comparing the aspect of the examined region of the heavens with Bremicker's excellent map, Hora XXI., he very soon found a star of about the 8th magnitude, nearly in the place pointed out by Le Verrier, which did not exist in the map. There was little or no doubt that this was the new planet; it was compared three times that night with a known fixed star, and an orbital motion was suspected; this was soon confirmed, and the observations of the two following days showed that its march was in the direction of, and nearly equal to, the prediction. Le Verrier's presumed diameter was $3''{\cdot}3$; and it may now be considered as established at $2''{\cdot}8$. But on the very first mensuration, with illuminated wires and a power of 320, the diameter was found to be $2''{\cdot}9$ by Professor Encke, and $2''{\cdot}7$ by Dr. Galle: when the field was enlightened, the same observers found the diameter to be $3''{\cdot}2$ and $2''{\cdot}2$ respectively,—but the later observations were made under unfavourable atmospheric conditions. This coincidence between *measure* and *estimate* is truly admirable, and shows the wonderful sagacity with which the existing data had been made use of. After an examination of the three days' observations, Encke pronounced that "the place of the planet agrees *within one degree;*" which correspondence between theoretic computation and actual observation must have been fully gratifying to Le Verrier; who, in concluding his memoir, observes:—"The error in my computation will be considered very trifling, when we reflect on the smallness of the perturbations from which the place of the planet had been inferred. This success allows of a hope, that after 30 or 40 years' observations of this new body, it may in turn be employed as a means of detecting the next which follows it in the order of distance from the sun. Thus, in the sequel, we shall unfortunately arrive at planets invisible on account of their immense distance from the sun, but the orbits of which will be correctly traced, in the course of centuries, by means of the theory of secular inequalities."

Thus then, by a profound computation based on very slight data, the closet Mathematician has been armed with more than magician's power, and not only pointed out the place in which a new planet must be found, but also limited the space in which to search for it, weighed its mass, determined its diameter, figured its orbital circuit, and numbered the years of its revolution around the central luminary! The confident temerity of the prediction was astounding, and staggered even the incredulous, for the most sanguine hope could hardly have expected so full a confirmation. "I cannot attempt to convey to you," said Mr. Airy, "the impression which was made on me by the author's undoubting confidence in the general truth of his theory, by the calm-

ness and clearness with which he limited the field of observation, and by the firmness with which he proclaimed to observing astronomers, *Look in the place which I have indicated, and you will see the planet well.*" And this wondrous and successful exertion of the powers of abstruse research was received with a burst of rapturous applause throughout the intellectual world; and numerous are the well-deserved honours showered on Le Verrier. His own Sovereign ordered his bust to be placed in the College of St. Lô, and decorated him with the cordon of the Legion of Honour; the King of Denmark enrolled him among the Knights of the Danebrog; and the Emperor of Russia conferred on him, by rescript, the second class of the Order of St. Stanislaus. In England he has been unanimously elected into the Royal and the Royal Astronomical Societies; and the former body awarded him the Copley Medal for 1846, their highest honour. Of the Astronomical Society's medal we shall presently speak.

Since this singular discovery, the stranger has been frequently scrutinized, both in our own country, and in many parts of the continent, and in North America. Thus the known boundaries of our planetary system have at once been nearly trebled, and a body added to it which, though a mere clod, and utterly useless in a worldly point of view, is a gem of the first order in confirming the truths of the Newtonian Doctrines. At present it has an apparently retrograde motion, amounting to two or three seconds of time daily; but its actual mean hourly motion in orbit must be about 12,000 miles, which is not half the movement of the once-designated *sluggish* Saturn, and is something less than one-sixth of our own rate of going. With a diameter of 43,000 miles, and a bulk nearly 200 times that of the Earth, it has a periodic revolution round the Sun of something less than a couple of centuries. To the slightly aided eye it appears as a star of the 8th magnitude, but a disc is raised under comparatively easy telescopic power. Our excellent friend Mr. Lassell, of Liverpool, has, moreover, viewed it with the largest *equatorial* instrument in existence, viz. a Newtonian reflecting telescope of his own construction, with an aperture of 24 inches, and a focal length of 20 feet. With this admirable tool, under powers varying from 316 to 567, he sees something crossing the disc, and also a probable satellite. The atmospheric conditions have not been favourable, but the streak is seen in the same direction, using two different mirrors, and by several observers. His own words are—"On the 3rd of October, at about $8\frac{3}{4}$ hours, I observed the planet to have apparently a very obliquely-situated ring, the major axis being seven or eight times the length of the minor, and having a direction nearly at right angles to a parallel of declination. At the distance of about three diameters of the disc of the planet northwards, and not far from the plane of the ring, but a little following, there was situate a minute star, having every appearance

of a satellite. I observed the planet again, about two hours later, and noticed the same appearance. * * * * With regard to the existence of the ring, I am not able absolutely to declare it, but I received so many impressions of it, always in the same form and direction, and with all the different magnifying powers, that I feel a very strong persuasion that nothing but a purer state of atmosphere is necessary to enable me to verify the discovery. Of the existence of a star having every aspect of a satellite, there is not the shadow of a doubt. Afterwards I turned the telescope to the Georgium Sidus (*Uranus*), and remarked that the brightest two of his satellites were both obviously brighter than this small star accompanying Le Verrier's planet." Since this communication was made by Mr. Lassell, the existence of the ring has been confirmed at Cambridge; but there are several points which await the further scrutiny of science.

Meantime we will submit an extract from the official Report which Professor Challis made to the Syndicate of that University, on the 22nd of March, 1847:—

"On Jan. 12, I had for the first time a distinct impression that the Planet was surrounded by a ring. The appearance noticed was such as would be presented by a ring like that of Saturn, situated with its plane very oblique to the direction of vision. I felt convinced that the observed elongation could not be attributed to atmospheric refraction, or to any irregular action on the pencils of light, because when the object was seen most steadily I distinctly perceived a *symmetrical* form. My assistant, Mr. Morgan, being requested to pay particular attention to the appearance of the Planet, gave the same direction of the axis of elongation as that in which it appeared to me. I saw the ring again on the evening of Jan. 14. In my note-book I remark, 'The ring is very apparent with a power of 215, in a field considerably illumined by lamp-light. Its brightness seems equal to that of the Planet itself.' On that evening, Mr. Morgan, at my request, made a drawing of the form, which on comparison coincided very closely with a drawing made independently by myself. The ratio of the diameter of the Ring to that of the Planet, as measured from the drawings, is about that of 3 to 2. The angle made by the axis of the Ring with a parallel of declination, in the south-preceding or north-following quarter, I estimated at 60°. By a measurement taken with the position circle on Jan. 15, under very unfavourable circumstances, this angle was found to be 65°. I am unable to account entirely for my not having noticed the Ring at an earlier period of the observations."

All this is most truly wonderful! In a region so awfully remote as nearly three thousand millions of miles from the grand central luminary, this extraordinary planet can receive but $\frac{1}{1300}$th part of the light and heat which we enjoy. The feeble effects of the sun there would almost point out that we have now reached the very utmost bounds of his influence: but should there still be another exterior planet,

which Neptune's backings and fillings may yet indicate, it will probably be nearly six thousand millions of miles distant, where the Cimmerian glimmer can only amount to $\frac{1}{40,000}$th of the earth's light. This interferes with that dogmatic fitness of things which of erst was pronounced by Sages, when Jupiter was shown to have four moons to compensate his far removal from the fountain of light, and Saturn, being then considered outside all, was allowed both a ring and satellites for the same object.

Such was the discovery, and such its conditions, as it met the public ear; and never was greater homage paid to cultivated thought than in the gratulations with which that public greeted the event. But as if the whole phenomenon was to be a startling affair, instead of the discovery being viewed with unmingled admiration, a new incident shook the opinions of men, and awakened considerable personality in certain quarters. It must be acknowledged that, considering national bias and excitement, something may be pleaded in extenuation. In brief, it appears that though the French geometer was so justly taking his triumphant lead, an English one was steering the same course with wet canvass close on his weather-beam; and who, had the look-outs been at their posts, would probably have been far ahead. Indeed, though without any intention of taking the shine out of the chace, it might be seen by all who had sailed for it, that there must inevitably be a close shave. This certainly took great numbers flat aback: the world at large were aware of the merit of Le Verrier, but until the unexpected announcements of Messrs. Airy and Challis, they knew nothing of the fact with which some few—ourselves among the number—were partially acquainted, viz., that a young Cambridge mathematician, hight J. C. Adams, as aforesaid, had been already led, by his own spontaneous thought and independent researches, not only to conclude that a planetary body, more distant than Uranus, actually existed, but also most skilfully to point out its habitat and features. Now as we conceive this point to be one of some historical and scientific moment, and as it has already been the cause of much difference of opinion, accompanied with some ebullition of small feeling, we will submit the leading facts of the case. We trust that a fair statement will put matters to rights,—for it were lamentable indeed that a new planet should prove an apple of discord, and disturb the harmony of astronomers: even the steps recommended by justice can be but ill-executed by petulance.

We will, therefore, proceed to examine the evidence, both formal and objective, begging the courteous reader to recognize the hackneyed adage—*audi alteram partem*—the while. Some of the points advanced may bear an inconsistent aspect; but all who bring their best judgment to bear on the subject, will easily discern how to correct the reckoning, and reduce it to the criterion of Truth.

Under this impression we shall, at the most knotty turns of the case, hand in the special test of official documents. Now it appears from the Report of the Astronomical Society for November, 1846, as well as that made by Professor Challis to the Syndicate of Cambridge, on the 12th of December following, that Mr. Adams had long formed the resolution of trying, by calculation, to account for the anomalies in the motion of Uranus: "he showed me," says Mr. Challis, "a memorandum made in 1841, recording his intention of attempting to solve this problem as soon as he had taken his degree of B.A. Accordingly, after graduating in January, 1843, he obtained an approximate solution by supposing the disturbing body to move in a circle at twice the distance of Uranus from the Sun. The result so far satisfied the apparent anomalies in the motion of Uranus, as to induce him to enter upon an exact solution." For this purpose he required a set of reduced observations, and applied to obtain them from Greenwich*, through the intervention of Mr. Challis; and this was the first distinct intimation to the Astronomer-Royal:—

"Cambridge Observatory, Feb. 13, 1844.

"A young friend of mine, Mr. Adams, of St. John's College, is working at the theory of Uranus, and is desirous of obtaining errors of the tabular geocentric longitudes of this planet, when near opposition, in the years 1818–1826, with the factors for reducing them to errors of heliocentric longitude. Are your reductions of the planetary observations so far advanced that you could furnish these data? and is the request one which you have any objection to comply with? If Mr. Adams may be favoured in this respect, he is further desirous of knowing, whether in the calculation of the tabular errors any alterations have been made in Bouvard's Tables of Uranus besides that of Jupiter's mass."

To this application, Mr. Airy immediately returned this reply:—

"Royal Observatory, Greenwich, 1844, Feb. 15.

"I send all the results of the observations of Uranus made with both instruments (that is, the heliocentric errors of Uranus in longitude and latitude from 1754 to 1830, for all those days on which there were observations, both of right ascension and of polar distance). No alteration is made in Bouvard's Tables of Uranus, except increasing the two equations which depend on Jupiter by $\frac{1}{50}$ part. As constants have been added (in the printed tables) to make the equations positive, and as $\frac{1}{50}$ part of the numbers in the tables has been added, $\frac{1}{50}$ part of the constants has been subtracted from the final results."

Dates now begin to be of paramount interest in the story, since a very discreditable rumour obtained, to which we must presently allude, on account of its notoriety. The next letter which appears, shows that Mr. Adams derived advantage from the communication; it is from Mr. Challis to the Astronomer-Royal:—

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* We should here state, that the first clear exhibition of the theory of Uranus was certainly made by the established routine operations at the Cambridge Observatory; and the beautiful reductions there tabulated, were eminently useful in all stages of Neptune's discovery.

"Cambridge Observatory, Sept. 22, 1845.

"My friend Mr. Adams (who will probably deliver this note to you) has completed his calculations respecting the perturbation of the orbit of Uranus by a supposed ulterior planet, and has arrived at results which he would be glad to communicate to you personally, if you could spare him a few moments of your valuable time. His calculations are founded on the observations you were so good as to furnish him with some time ago; and from his character as a mathematician, and his practice in calculation, I should consider the deductions from his premises to be made in a trustworthy manner. If he should not have the good fortune to see you at Greenwich, he hopes to be allowed to write to you on this subject."

To this Mr. Airy appends a remark, "On the day on which this letter was dated, I was present at a meeting of the French Institute." This incidental observation, slight as it is, has raised a bubble in the minds of some of the magnates of the periodical press, and several of their followers. A sturdy assailant took the field in the Mechanics' Magazine, and unprovided with either proof or probability, trumpeted the delinquency of the Astronomer-Royal to the world: how that he, sojourning in Paris, did then and there most imprudently, as well as naughtily, let the cat out of the bag, supplied Le Verrier with Adams's work, and informed the wondering Frenchmen all about the new planet. Yet this Seer cannot have had the slightest basis for so bare-faced an assertion; for from the incontrovertible internal evidence of the Report read to the Astronomical Society, and which we are quoting, the Astronomer-Royal must be acquitted of the silly but foul charge by every pure-minded investigator. On receiving a copy of Le Verrier's Memoir, on the 23rd or 24th of June, of the following year, he thus returned his acknowledgements:—

"Royal Observatory, Greenwich, 1846, June 26.

"I have read, with very great interest, the account of your investigations on the probable place of a planet disturbing the motions of Uranus, which is contained in the Compte Rendu de l'Académie of June 1; and I now beg leave to trouble you with the following question. It appears, from all the later observations of Uranus made at Greenwich (which are most completely reduced in the Greenwich Observations of each year, so as to exhibit the effect of an error either in the tabular heliocentric longitude, or the tabular radius vector), that the tabular radius vector is considerably too small. And I wish to inquire of you whether this would be a consequence of the disturbance produced by an exterior planet, now in the position which you have indicated?

"I imagine that it would not be so, because the principal term of the inequality would probably be analogous to the Moon's variation, or would depend on sin 2 $(v-v')$; and in that case the perturbation in radius vector would have the sign — for the present relative position of the planet and Uranus. But this analogy is worth little, until it is supported by proper symbolical computations."

Now here there is not the most distant allusion to Mr. Adams, which must have been the case, had the writer committed himself at Paris, as so deliberately alleged.

Most of Adams's friends were staggered by the boldness of his problem, as announced by so young a mathematician: and though he showed that his hypothetical body would satisfy all the anomalies in the most trustworthy observations of Uranus, still, under what they deemed a justifiable scepticism, they lost the moment for victory. Had there been hope and confidence Le Verrier and Adams must have changed places; but while the former was brought out in full daylight, the latter was shrouded in secresy. Though the basis was sound, there was not sufficient faith: so that this, being the first instance of a solution of the abstruse and difficult analytical investigation of the inverse problem of perturbations*, was not made public. It was unfortunate that it appeared to the Plumian Professor as "so novel a thing to undertake observations in reliance upon merely theoretical deductions, and that while much labour was certain, success appeared very doubtful," that he neither engaged in the pursuit himself, nor afforded to others the means of doing so. Under a similar misgiving, the Astronomer-Royal says, that when he found Le Verrier's place for a disturbing planet was the same, to one degree, as that given by Mr. Adams's calculations, which he had perused seven months earlier, he began to look to it. "To this time," he says, "I had considered that there was still room for doubt of the accuracy of Mr. Adams's investigations; for I think that the results of algebraic and numerical computations, so long and so complicated as those of an inverse problem of perturbations, are liable to many risks of error in the details of the process. I know that there are important numerical errors in the *Mécanique Céleste* of La Place; in the *Théorie de la Lune* of Plana; above all, in Bouvard's first *Tables of Jupiter* and *Saturn;* and to express it in a word, I have always considered the correctness of a distant mathematical result to be a subject rather of moral than of mathematical evidence. But I now felt no doubt of the accuracy of both calculations, as applied to the perturbation in longitude. I was, however, still desirous, as before, of learning whether the perturbation in radius vector was fully explained."

The later remark brings us upon another point in this curious and eventful bit of history. When Mr. Adams made his first statement, Mr. Airy requested to know, "whether the assumed perturbation will explain the error of the radius vector of Uranus?" To this inquiry,

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* The inverse ratio of perturbations, is that in which the computations may be made from apparently anomalous motions in the body under influence, and not from the known attractions of the body influencing: in other words, from known disturbances of a planet in known positions, to find the place of the disturbing body at a given time. Here, as the reason necessarily bears from the effect to the cause, and not from the cause to the effect, for that was unknown, the problem was one of extreme difficulty, and heretofore—as far as we know—untried.

from some cause or other unexplained, no immediate answer was returned: but on asking Le Verrier the same question, he received a ready and precise reply,—the observed errors of the radius were corrected in his orbit, that they corrected themselves, without any direct consideration; and he added, "*Excusez moi, Monsieur, d'insister sur ce point. C'est une suite du désir que j'ai d'obtenir votre suffrage.*" We can readily allow for the cautious feeling which made the question of the radius vector so strongly insisted upon, as a crucial instance of the actual strength of the supposed discovery; and it might have been answered in some way or other. But this ought not to have been an obstruction, especially as Adams had eliminated all the errors of longitude, which was his principal object; and it seems that he actually employed a method of calculation which required him to compute the co-efficients of the expression for error of radius vector, *before* computing the co-efficients of the expression for error of longitude. It is, therefore, to be regretted that this co-ordinate should have impeded the Cambridge correspondence, by giving, however unintentionally, the appearance of a slight to the referee.

The plot was now thickening. At a meeting of the Board of Visitors of the Royal Observatory at Greenwich, the Astronomer-Royal alluded to the impending discovery of a new planet, since there was a singular accordance between the investigations of Adams and Le Verrier. From this remark,—and here we speak advisedly, though not in accordance with M. Arago's argument,—originated the eloquent expression of Sir John Herschel to the British Association, at Southampton, on the 10th of September. Having observed that the last year had given another new planet (*Astrea*) to our system, he added,—"It has done more: it has given us the probable prospect of another. We see it as Columbus saw America from the shores of Spain. Its movements have been felt, trembling along the far-reaching line of our analysis, with a certainty hardly inferior to that of ocular demonstration." And the same discussion led Professor Challis to contemplate a search for the suspected disturber,—a search not before thought of.

The Astronomer-Royal transmitted to Cambridge suggestions for the examination of a region of the heavens 30° long, in the direction of the ecliptic, and 10° broad, having the theoretical locus of the planet at its centre: and at the same time he made a liberal offer of assistance, even at his own cost, the which, to our surprise, was not accepted. A modification of the suggested plan was adopted, and 3,150 positions of stars were recorded; but it so happened that this was like sweeping a large Turkey carpet in quest of a lost diamond, which might have been detected by its inherent brilliance on the spot where it was dropped; and though this course was adopted to prevent ultimate disappointment, yet a careful eye-scrutiny with the powerful telescope employed, must have pro-

duced the planet in the early part of August. Mr. Adams had found the mass to be about three times that of Uranus, and had thence inferred that the brightness would not be below that of a star of the 9th magnitude; but his consequent request that the planet might be sought for by its physical aspect, was neglected. This is matter of regret, since, from the surpassing interest of the question, it ought to have been fished for nine months before, namely, in October, 1845, when both the prediction and the detection would infallibly, and without competition, have fallen to Cambridge; and England would have enjoyed an incontestable right to a sort of astronomical feat which, great as she is, she is most in want of. These are the elements upon which the scrutiny was eventually conducted:—

	Hypothesis I. $\left(\frac{a}{a^1} = 0{\cdot}5\right)$	Hypothesis II. $\left(\frac{a}{a^1} = 0{\cdot}515\right)$
Mean Longitude of Planet, 1st Oct., 1846	325° 8′	323° 2′
Longitude of Perihelion	315 57	299 11
Eccentricity	0·16103	0·12062
Mass (that of Sun being 1)	0·00016563	0·00015003

Such being the conditions of the case, we must proceed to consider them, and we trust at least to bring impartiality to bear. According to the Astronomer-Royal's incontrovertible evidence, no doubt can be entertained of Adams's being *de facto* the first to predict the existence and locus of a new planet. Such a body was *à priori* probable; and the skilful geometer showed, by giving all the possible elements (*node and inclination out of the question*), and the place at a given time,—that such a body would satisfactorily account for the errors observed in the motions of Uranus. Why he did not explain Mr. Airy's query about the radius vector is not in evidence; but the errors of that condition are readily deducible from both the above-cited hypotheses. It is also now quite clear that the Cambridge astronomer had actually got sights of the planet on the 4th and the 12th of August, seven weeks before Dr. Galle's discovery of it; but he assuredly was not aware of it; for he says—"after four days of observing, the planet was in my grasp, *if I had only examined or mapped the observations*,"—"my observations would have shown me the planet in the early part of August, *if I had only discussed them*." "I lost the opportunity of announcing the discovery, *by deferring the discussion of the observations*, being much occupied with the reductions of comet observations, and *little suspecting that the indications of theory were accurate enough to give a chance of discovery in so short a time*." That the observer was not really aware of the planet's having been caught, and that he did not even expect it was, is evident from the following letter, written by him to the Astronomer-Royal:—

"Cambridge Observatory, Sept. 2, 1846.

"I have lost no opportunity of searching for the planet; and, the nights having been generally pretty good, I have taken a considerable number of observations: but I get over the ground very slowly, thinking it right to include all stars to 10–11 magnitude; and I find, that to scrutinise, thoroughly, in this way the proposed portion of the heavens, will require many more observations than I can take this year."

And he further declares, that on receiving tidings of the planet's discovery at Berlin, he was so much impressed with the sagacity and clearness of M. Le Verrier's limitations of the field of observation, that he instantly changed his plan of observing, and noted the planet, as an object having a visible disc, *on the evening of the same day!* Indeed the Professor's own statements open the door of controversy, both as to the actual discovery and the precedence of publication: "A comparison," he says, "of the observations of July 30 and August 12, *would*, according to the principles of search which I employed, have *shown me the planet*. I did not make the comparison of it till *after the detection of it at Berlin*, partly because I had an impression that a much more extensive search was required *to give any probability of discovery*, and partly from the press of other occupations." And though he descried the planet on the 29th of September, he merely directed his assistant to write against that *star*, it "*seems to have a disc*,"—so that uncertainty reigned till the Berlin news arrived on the 1st of October, when "all was light."

As M. Le Verrier's memoirs were under publication, it became necessary to print also Mr. Adams's calculations and formulæ; but as an unavoidable delay must occur in the medium which he resorted to, Lieutenant Stratford, the able Superintendent of the Nautical Almanac, came to his relief, by a timely offer of printing the paper as a supplement for the Ephemeris of 1851; but with a view of circulating a number of copies of it forthwith. This will be best told in the Lieutenant's official notice, prefixed to the paper:—

"This paper was communicated by the Author to the Royal Astronomical Society, and was read to that body, at their ordinary meeting, on November 13, 1846. The press of the Society being engaged on an extensive paper, on the longitude of Valentia, by the Astronomer-Royal, and it being deemed of national importance that Mr. Adams's paper should be submitted to the world without loss of time, application was made to Capt. W. H. Smyth, R.N., President, and to the Rev. R. Sheepshanks, Secretary, of the Society, who, with their usual promptitude and zeal, granted permission for the immediate printing and publishing of the paper by the Nautical Almanac Office; and it is under these circumstances that the investigations of Mr. Adams first appear as an extract from the Appendix to the Nautical Almanac for 1851.

"W. S. Stratford, Superintendent of the Nautical Almanac.

"Nautical Almanac Office, 3, Verulam Buildings, Gray's Inn, London.

"December 31, 1846."

The publication of his method showed that Mr. Adams had not

arrived at his conclusions by rough estimation, or graphical leaps; and that while his advance was cautious, his steps were masterly and refined. But though he was thus raised in the public opinion, it did not shake Le Verrier's claim a whit. The completeness of that thorough geometer's work, added to the orderly decision with which he conjured astronomers to mark down the quarry, excite our warmest admiration. No petty jealousies ought to defile this feeling. By all the rules of fair adjudication, the noble prize is his; nor has anybody tendered it in more appropriate and truly liberal terms than his excellent competitor. "I mention these dates," says Mr. Adams, "merely to show that my results were arrived at independently, and previously to the publication of those of M. Le Verrier, and not with the intention of interfering with his just claims to the honour of the discovery; for there is no doubt that his researches were first published to the world, and led to the actual discovery of the planet by Dr. Galle, so that the facts stated above cannot detract, in the slightest degree, from the credit due to M. Le Verrier." This is somewhat of a contrast to the virulent conduct of certain French journalists, and the frothy excitement of others on record, who cannot plead the proverbial hastiness of youth in extenuation. Even the liberal M. Arago, albeit delighted at our system being enriched with a new constituent, most precipitately and harshly enounced—"that Mr. Adams is not entitled to the *slightest allusion* in the history of the discovery!" (*Comptes Rendus*, 19 *Oct.*, 1846.) But M. Biot, on the contrary, giving full credit to our countryman, thus speaks,—"Je ne parle pas ici d'après ce sentiment d'égoïsme géographique, appelé si improprement du patriotisme. Les esprits voués à la culture des sciences ont, à mes yeux, une commune patrie intellectuelle, qui embrasse tous les degrès d'élévation du pôle." This is the language of true philosophy: it is certainly no disparagement of Adams's claims to distinction, to say that the splendour of this discovery belongs to Le Verrier; nor does a full acknowledgement of the merits of the one detract from those of the other.

But the sound and brilliant title of Mr. Adams has been rather tarnished than otherwise, by the well-intended clamour of over-zealous friends. Of this a remarkable instance occurred at the Tercentenary dinner of Trinity College, on 22nd December, 1846. On this occasion, the Master declared to the assembled guests—"If they needed anything to remind them of that (*the necessity of Colleges maintaining a community of interest*), they would find it in the reflection that the great discovery in Astronomy by which this age would be known, was due to one of their friends on the other side of the wall*. *Loud cheers*." This, like the food and the wine, was willingly

* Those unacquainted with the topography of Cambridge may be told, that a high wall divides the Colleges of Trinity and St. John's.

swallowed; but we are compelled by the facts before us, to consider it as a decided over-statement, or rather, a momentary ebullition. Had not Le Verrier's announcement of the planet's locus appeared, it is not improbable that no large telescope would have been directed to the heavens in search of it. To be sure, Mr. Adams *might* have still worked at his theory—he *might* have insisted on its publication in its then state—he *might* have published it himself—or he *might* have communicated his elements to the Astronomical Society, &c., &c.; but this is quite irrelevant, for none of these things were done, and no telescope was ever turned to the sky till the publication of Le Verrier's results *forced* it to be done. We cannot therefore understand—whatever claim may exist on other scores—how the discovery of the planet was "due" to Mr. Adams's researches. Assuredly the contrary is the case. Galle looked for it, and found it, by Le Verrier's instructions solely. No one can show that he looked for it purely and simply by Adams's, nor was it begun to be looked for here before we had a knowledge of Le Verrier's conclusions. Q. E. D.

Under such unquestionable facts, the debate of absolute priority is one of grave import, and must ultimately depend upon what may be deemed the publication of this wonderful problem. A large and rather influential party adhere to a lop-sided decision of the deplorable contest between Leibnitz and Newton, and cite all sorts of unilateral incidents, which may be deemed rather exceptions than rules: but in the present "enlightened" day, one would no more think of reverting to logogriphes than of sending a parcel by pack-horse to Bristol. The custom of Galileo and Co. would be about as antiquated as keel-hauling a sailor for rapping out an oath would be. We give Mr. Adams the full benefit of that axiom in law which decides, that such evidence as a jury may have by their private knowledge of facts, has as much right to sway their judgment, as the written or parole evidence which is delivered in court: but we also recollect that in law, ever since the dispute between Euphorbus and Menelaus, it is contested whether he that first wounds a beast that is classed among the *feræ naturæ*, or he that kills it, were to bear off the spoil and quarry. In the case before us, it seems that Adams shot at the stranger, but Le Verrier brought him down. It were better, therefore, that they remain Arcades-ambo through successive ages, than attempt a division of interests. Lord Mansfield, however, if we may judge by his celebrated decision in Dollond's achromatic-telescope trial, would have declared for the Frenchman: when it was pleaded as an objection to Dollond's patent, that Dr. Hall had made the same discovery many years before, his Lordship held that as the public were not acquainted with the fact, Mr. Dollond must be regarded as the true inventor: he was not only a discoverer of it as well as Dr. Hall, but being the *first Publisher*, was fully entitled to all the benefit. So

Waring also states "that person is the first discoverer who first *publishes* his discovery;" but if from diffidence, design, or carelessness, he does not make his discovery known, then such *lache* establishes a second-comer in equal rights.

These considerations involve a sort of necessity to offer a few words on the recent conduct of the Royal Astronomical Society, in regard to the non-award of their medal in the present unprecedented instance; an instance such as can seldom occur, and probably never will recur again. It is well-known, that it is in the power of the Council of this meritorious Society, to confer one gold medal annually upon the most important astronomical discovery of the year. But to prevent the award of medals to unimportant communications, a bye-law—and perhaps a wise precaution—requires that there shall be a majority of votes of three to one in order to give the prize. On this occasion there was a serious difficulty: two opinions prevailed—first, that a medal should be voted to M. Le Verrier alone; secondly, that unless a medal were also given to Mr. Adams, a great injustice would be done. But the whole imbroglio will best appear, on citing the official minutes of their Anniversary Meeting: and thus they run—

"Friday, February 12, 1847.

"Capt. W. H. Smyth, R.N., President, in the Chair.

"Were it intended to describe the results of the century instead of the current year, the subject to which your Council now come would lose none of its prominent interest. The prediction of a new planet, on grounds derived from calculation only—the fulfilment of that prediction—the attainment of the solution of the inverse problem of perturbation—mark the years 1845 and 1846 with an importance which belongs to no period except that of the announcement of the theory of gravitation and of the publication of the *Principia*.

"The circumstances under which the discovery was made add to the interest of the question, by throwing difficulty in the way of the settlement of points of opinion connected with its history. And the embarrassment is materially increased by the necessity of deciding national rights, and of guarding against the undue influence of national feeling.

"The facts connected with this singularly splendid triumph of mind over matter have been much discussed, and are now fully published. The statement made to this Society by the Astronomer-Royal in November, the memoirs of M. Le Verrier, the memoir of Mr. Adams, and the statements made by Mr. Challis, and in various numbers of the *Comptes Rendus*, have put our Fellows in such possession of the absolute circumstances of the case as renders any detailed account of them unnecessary in this Report. It fortunately happens that there is no one disputed fact; but upon the construction of the facts, and upon the meaning of words, there are differences of opinion, at least as wide as those which have always existed upon the great question of the claims of Newton and Leibnitz to the invention of fluxions.

"In one thing there is general agreement, namely, in giving both to M. Le Verrier and Mr. Adams the highest order of praise and admiration. As soon as they are compared, all manner of opinions are found to prevail as to their *relative* positions; but on the absolute character of the rank taken by the labours of both in the history of astronomical discovery there can be but one feeling.

"Under these circumstances it will be matter of regret, but hardly, all things considered, one of astonishment that your Council has not been able to give any verdict upon the disputed matters of opinion, nor to afford, to any conclusion, the sanction which would be considered as implied in the award of a medal to M. Le Verrier, to Mr. Adams, or to both. Such a tribute is not needed by either; and your Council distinctly request it may be understood, that in making a statement of the circumstances under which they have failed to arrive at a decision, they are simply accounting to their constituents for their own conduct, and not intending to draw any conclusion upon the controverted opinions. Perhaps there is not one among them who does not, more or less, censure the collective body to which he belongs for not adopting a positive course: while, perhaps, there are very few indeed who could agree upon any one mode of proceeding. And it is by no means improbable, that the same general wish that something had been done, and the same disagreement as to what it should be, which has prevailed in the Council, would also prevail in the Society.

"By our bye-laws, only one medal can be given in any one year; but it is in the power of a General Meeting, at the proposal of the Council, to suspend or abrogate any bye-law. Again, by the same laws, all propositions for the award of medals must be made and seconded in November, and taken into consideration in January. That no possible view of the case might be precluded from discussion, the individual members of the Council, with whom every such proposition must originate, took care that the list of those nominated for the medal in November last should contain all the names which could by possibility come into question.

"The first point of discussion was, whether it would be expedient to recommend the General Meeting to suspend the existing bye-law, and to give the power of awarding more than one medal. This, it is very obvious, has in itself a question of expediency, totally independent of the particular circumstances under which the permission is sought: and a motion was made to the effect that such a course was not expedient. This motion was carried; and as it may be presumed that the grounds on which it was brought forward are those on which it was carried, the Council think it right to state those grounds.

"In carefully guarding the decisions of the medal, by placing such awards wholly in the hands of the Council, and declaring that no medal shall be given by the Society at large, the latter body has made a standing confession of the very obvious truth that a large assembly of men, interested in astronomy in very different ways, and to very different extents, does not form so proper a court for the decision of delicate questions of personal merit as a smaller body chosen by themselves, out of all whose occupations will allow of their attendance, as a specified number of those who are best qualified to conduct the affairs of the Society. If we consider how many propositions

it is open to any Fellow of the Society to make, and how few are made except through the Council, it would appear that the general feeling is, that the letter of the law respecting medals is only the expression of the spirit in which the Society desires that its business should be conducted.

"It was contended that this spirit of our laws would be violated, to the introduction of every disadvantage which those laws were intended to avoid, if a more than usually difficult question were submitted to the Society, of the very kind which the Society had peculiarly delegated to the Council, even in the ordinary and easier cases. Taking it for granted that the existing law was adopted for wise reasons, it was urged that it would be highly improper to force upon the general body the public discussion of the nicest question of relative merit which has arisen for more than a century; and that it might reasonably be expected that the extremes of opinion found to exist in the Council might be taken as a low estimate of those to be looked for in a larger body. The motion founded upon this view of the case was carried.

"It being then decided that no recommendation to depart from the usual course should emanate from the Council, the question to whom the *one medal* should be awarded necessarily followed. The claim first considered was that of M. Le Verrier, whose name stood first on the list. This medal being, under the circumstances, an expression of opinion upon a matter likely to be long under discussion, or at least certain to be so interpreted both at home and abroad, it seems to have been thought by several that an award to M. Le Verrier, unaccompanied by another to Mr. Adams, would be drawing a greater distinction between the two than fairly represents the proper inference from facts, and would be an injustice to the latter. Accordingly, on a ballot being taken, it appeared that the majority in favour of the proposition was not sufficient to carry it, the bye-laws requiring that no medal should be awarded upon any majority of less than three to one. No award could therefore be made; and the Council can only conclude upon this matter, that the differences of opinion prevailing among the members render it impossible for them, as a body, to offer any statement upon the controverted points of the question.

"Perhaps it would not be improper to add, that in a question in which a French and English claim are mixed, in a manner which requires a perfect absence of national feeling rightly to settle, it is not to be regretted that this Society should thus have been compelled, by the action of its own laws, to refer the decision to the astronomers who are of neither of the nations thus placed in opposition."

"The Report having been read, it was Proposed by Mr. R. Taylor,—Seconded by Captain Sir John Ross: 'That the Report of the Council now read be received and adopted, and that it be printed and circulated in the usual manner.'

"Proposed in amendment by Mr. Babbage,—Seconded by Dr. Fitton: 'That this Meeting express their deep regret that the Council have not awarded the Society's medal to M. Le Verrier, for his publication of the greatest astronomical discovery of modern times.'

"This amendment was negatived.

"A second amendment was Proposed by Lieut. Raper, R.N.,—Seconded by Capt. Bethune, R.N.: 'That it is the opinion of the Meeting that the unprecedented discovery of a new planet by theoretical researches, and the acknowledged title of M. Le Verrier to the honour of that discovery, demand for him some special mark of the approbation of this Society: that it be recommended to the new Council to convene a Special General Meeting of the Society, on as early a day as may be convenient, for the purpose of suspending Articles 2, 3, and 4, of Section 16 of the Bye-laws; and that the printing of the Report be deferred till the subject shall have been brought under the consideration of such Special General Meeting.'

"This amendment was also negatived.

"A third amendment was Proposed by the Rev. R. Sheepshanks,—Seconded by Mr. Drach: 'That a Special General Meeting be called to consider the propriety of granting a medal to M. Le Verrier, for his researches respecting the planet exterior to Uranus; a medal to Mr. Adams for his researches on the same subject.'

"This amendment was also negatived.

"A fourth amendment was Proposed by the Astronomer-Royal,—Seconded by Dr. Lee: 'That a Special General Meeting be called after the ordinary Meeting on March 12, to consider the following resolutions:—

"'That so much of the bye-law as relates to the number of medals which may be adjudged in any one year, the time of giving notice of the proposal for a medal, the time of adjudging the medal, and the time of presenting the medal, be suspended *pro hâc vice;*

"'That the Council be authorised to award two (or more) medals, if they shall deem it expedient to do so;

"'That the award of the Council be communicated to the Society, and that the medal or medals be presented at the ordinary Meeting of April 9.'

"This amendment was carried.

"Proposed by Mr. De Morgan,—Seconded by the Rev. R. Sheepshanks: 'That this Meeting be adjourned to Saturday, Feb. 13th, at 2 o'clock.'"

SATURDAY, FEBRUARY 13, 1847.

"The Society met at 2 o'clock, according to adjournment, Captain W. H. Smyth, R.N., President, in the Chair.

"Proposed by Mr. De Morgan,—Seconded by Sir J. Ross: 'That the Report of the Council read yesterday be received and adopted, and that it be printed and circulated in the usual manner, with an account of the proceedings of the Meeting annexed.'

"Proposed in amendment by Mr. Babbage: 'That this Meeting do adjourn to a day to be named at the next General Meeting.'

"This amendment was not seconded. The original Motion was then put and carried.

"Proposed by Lieut. Raper,—Seconded by G. B. Airy, Esq.: 'That the thanks of the Meeting be given to the President for his conduct and temper in the Chair, during the continuance of this Meeting.'

"Carried unanimously."

The whole of these proceedings were conducted with sense and good

feeling, although the combatants were at times rather warmly engaged; the chief interlocutors being severally under the full conviction of a righteous cause. During the contest, all comparison between the respective merits of Adams and Le Verrier was so discouraged, that the few remarks which were uttered of the kind, fell dead. The bone of contention was, therefore, as to whether the bye-laws should be tampered with, or any interference be allowed in the Council's decision; that body having, as judges, acted to the best of their abilities, which is the only obligation they are morally placed under. When the Special General Meeting assembled to bring Mr. Airy's propositions under discussion, on March 12th, an amendment was proposed by Mr. De Morgan, "That on taking all the circumstances into consideration, and particularly the existing differences of opinion on the subject, it is not expedient to propose to the Council to reconsider the subject of the medal." This amendment was carried; and the Meeting broke up with the highest respect for the rival candidates. All hands were fully persuaded that the orbit of a planet exterior to Uranus had been defined, and its locus predicted, by Adams, with sufficient correctness for picking it up, in November, 1845, had it been duly looked after; but that Le Verrier, on quite independent grounds, stepped in and triumphantly bore it off in September, 1846. This is altogether a far more praiseworthy and gratifying specimen of competition between France and England, than some which might be cited.

We were about to close our lucubration, when we suddenly recollected that the style and title to be assigned to the new planet, have excited almost as much fermentation as its discovery had evulgated. Shakspeare, it is true, somewhat temerariously demands "What's in a name?" but astronomers think, with Pythagoras, that "it requires much wisdom to give right names to things." Hence the mighty turmoil which still disturbs the atmosphere of science; while some wish the discoverer's name to be attached to any newly-detected celestial body, a still larger class are clamorous for retreating upon classical mythology, as neutral ground in unison with the existing order of nomenclature. Ophion, Gallia, Atlas, Chronos, Gravea, and Oceanus, were severally proposed and rejected. Janus was rather favourably received, on account, it is insinuated, of one face of the *bifrons Deus* representing the mathematical, and the other the physical discoverer. A friend of elegant mind thought Minerva would be appropriate, despite of a little Pallas being already installed: and Hyperion, the offspring of Uranus and Terra, presented himself, not as the Sun or the Sun's father, but in capacity of Ὑπεριων, the *Transcender*, or more literally *above us going*. An Oriental scholar suggests a higher flight into antiquity, and there picking up Sanchoniatho's Elioun, the Hypsistus of Philo-Byblius, because he was the reputed parent of Uranus: but in quoting these gentlemen, we trust we

are not poaching on Ephraim Jenkinson's ground, or otherwise disturbing the shade of Goldsmith. Le Verrier himself sanctioned Neptune, the designation conferred by the *Bureau des Longitudes* at Paris; and the sea-deity instantly gained the largest number of votes, especially as the symbol was a trident made from a monogram of the initials of the French geometer. So the symbol of Uranus identifies Herschel; and by such course the discoverer of a planet will ever be held in honour and remembrance, whatever may be the appellation of the discovery.

But though most of the *e merito* astronomers signified their adhesion to Neptune, he was not allowed to walk the course. A terse northern Professor, overlooking the marine deity's alliance with us in ruling the waves, thus perorates:—"The god is degraded, in the eyes of a Briton at least, by the disturbing influence of low and vulgar associations. For who can hear of Neptune as the name of the new planet, without being reminded either of the wooden sea-god that he has seen, trident in hand, in the poop of many a vessel, or of his living representative in the person of a sailor at the ceremony of Crossing the Line, or in some pantomime at Sadler's Wells; or, it may be, of some Newfoundland dog who rejoices in the name of Neptune?" And this is all which a British Professor knows of Neptune! The unkindest cut of all, however, was given by our gifted friend M. Arago, who publicly pledged himself (*je prends l'engagement*), whatever might happen, not to call the stranger by any name except that of Le Verrier: a decision at which M. Le Verrier, who was present at the sitting of the Academy, says he was somewhat startled (*j'ai été un peu confus*). United Service readers to a man will, we expect, stick to Neptune and the Trident.

Thus endeth our story of the new Planet. To be sure Mrs. Borron, of Croydon, has publicly stepped forth and insisted that Neptune is not the body sought for by Le Verrier's investigation, but a planet which happened accidentally to be in the field of the telescope when Dr. Galle made his scrutiny. Since this assertion was openly divulgated, our brethren on the other side of the Atlantic have arrived at the same conclusion, and have, moreover, supported Mrs. Borron's paradox by $x + y - z$. There are certainly perturbations still to account for; and the mean distance of Neptune proving to be much less than the limits assumed, may indicate a change in their very character. Professor Peirce communicated to the American Academy of Sciences, 16th March, 1846, the computations of Mr. Sears C. Walker, who had detected a missing star in the *Histoire Céleste Française*, observed by Lalande, on the 10th of May, 1795, near the path of the planet Neptune, at that date, which may have possibly been the planet in question. Mr. G. P. Bond joined in the scrutiny of all the data; and the conclusion which these gentlemen have arrived at is, that *the planet Neptune is not the planet to which geometrical analysis had directed the telescope.*

Let the whole corps of Geometers look well to this, and unveil the happy accident to which the discovery of Galle is owing; let them tell how queerly Lalande allowed Neptune to slip through his fingers, after catching him on the 8th and 10th of May; and let them revise the now-faulty elements of the complicated motions before them.